cidentally learned that the only species of *Scyllarus* known to Fabricius* in 1775, when he first made known that genus, was the *S. arctus* —the *Cancer arctus* of Linnæus. That, being the only species, is necessarily the type, and, therefore, the name *Scyllarus* must be retained for it. The early carcinologists (Latreille, White) correctly recognized the type. Nevertheless, the *S. arctus* was taken as the type of a new genus—*Arctus*—and the name *Scyllarus* was reserved for the "*Sc. sculptus, latus, squamosus, equinoxialis, Haanii, Sieboldi,*" by Dana in 1852. He was doubtless influenced in this respect by the consideration that the *arctus* was the only species of its genus known to him, while most belonged to the other one. All succeeding carcinologists have followed him, and, indeed, the family is one of the very few for which a classification proposed nearly half a century ago has been retained intact to the present time, new species only having been added meanwhile. However, the necessity for a change will be recognized by almost every zoologist, and the sooner it is made the better it will be. I, therefore, propose to restore *Scyllarus* to the typical species, and to give the new name *Scyllarides* (*Scyllarus* with the Greek patronymic termination *-ides*) to the bereft genus. *Scyllarides* may be typified by the *S. æquinoctialis* (*Scyllarus æquinoctialis* of Nicolaus Tönder Lund).†

According to Dr. Ortmann (Zool. Jahrb., Syst., 268, X., 1897), there are five well defined species of *Scyllarides*—*squamosus*, *latus*, *haani*, *æquinoctialis* and *elizabethi*.

THEO. GILL.

WASHINGTON.

LAMARCK AND 'A PERFECTING TENDENCY.'

PROFESSOR JOHN GARDINER has done well to recall the fact that the chief factor in evolution, according to Lamarck, is not the so-called 'Lamarckian factor,' but '*a perfecting tendency.*' Lamarck's Histoire Naturelle is in perfect accord with his Philosophie Zoologique, as interpreted by Professor Gardiner. Lamarck thus describes his two factors: (1) '*Composition progressive,*' '*progression,*' '*plan de la nature,*' "*pouvoir qui tend sans cesse à compliquer l'organisation, à accroître le nombre et le perfectionnement des facultés,*" "*cause première et prédominante.*" (2) "*La cause accidentelle n'ayant pu altérer la progression en question, que dans des particularités de détail, et jamais dans la généralité des organisations.*"

The editors of the second edition of the Histoire Naturelle add a foot-note (Vol. I., p. 114) which concisely states Lamarck's position: "Il y a donc, d'après Lamarck, deux causes toujours agissantes sur les animaux, l'une qui tend à les perfectionner d'une manière uniforme dans leur organisation, l'autre modifiant irrégulièrement ces perfectionnements, parcequ'elle agit selon les circonstances locales, fortuites, de température, de milieu, de nourriture, etc., dans lesquels les animaux vivent nécessairement."

Lamarck repudiates the '*échelle graduée*' of Bonnet, and claims there is no identity between it and his '*composition progressive.*'

C. O. WHITMAN.

*Systema Entomologiæ, p. 413, 1775.

† The proper authority for the species (generally known as '*S. æquinoxialis* Fabr.') has been given by Miss Rathbun in the Annals of the Institute of Jamaica (I., 43). The excellent memoir of Lund (Om Slaegten Scyllarus < Skrivter af Naturh. Selskabet, II., p. 17-22, 1793) has been ignored by almost all others. It was referred to by White, but the references to Lund were mostly given after those to Fabricius.

SCIENTIFIC LITERATURE.

Recent and Coming Eclipses. By SIR NORMAN LOCKYER, K.C.B., F.R.S. Macmillan & Co. 1897.

This volume, consisting mainly of articles which have appeared from time to time in current periodicals, is issued with a view to supplying the general reader with information regarding the latest phases of the chief eclipse problems.

The treatment divides itself into two parts. The earlier chapters of the work contain elementary explanations of the theory of eclipses, and that of the instruments used in their observation. The spectroscope in its various forms is discussed in detail, and much stress is laid on the efficiency of the slitless spectroscope or 'prismatic camera.' The application of this

instrument to many of the problems of solar physics is dwelt upon at length. A chapter is also devoted to the various simple observations which can be made without the use of elaborate apparatus. Following this preliminary discussion is an account of the eclipse expedition to Kiö Island, with a description of the arrangement of the camp and apparatus, and an account of the development of the latent observational powers of the officers and crew of H. M. S. 'Volage,' which had been detailed to assist in the expedition. Then comes the story of clouds, failure and the retreat. A chapter is now devoted to the success at Novaya Zembla, where Mr. Shackleton succeeded in obtaining the spectrum of the chromosphere by means of a prismatic camera. This finishes what has been referred to as the first part of the work.

What follows is devoted to the bearing of eclipse observations up to date upon the question of the composition and distribution of the solar atmosphere. It is stated that the 'flash spectrum' of the chromosphere is radically different from the ordinary absorption spectrum with which we are familiar, and that therefore the chromosphere is not the seat of most of the absorption. Comparisons are made with arc and spark spectra and that of 'hot stars' with a view to showing that the chromosphere is hotter than the absorbing media, which must therefore be situated higher up in the solar atmosphere. The step from this proposition to dissociation is a short one, and, with the satisfied conclusion that "The eclipse work strengthens the view that chemical substances are dissociated at solar temperatures," the author closes his book.

In brief, it may be said that the features of the work are the stress laid upon the importance of the prismatic camera in eclipse work, the account of the volunteer corps of the 'Volage,' and the exposition of the vindication of the dissociation hypothesis by all the phenomena of solar and stellar spectroscopy.

While there is no denying the fact that in the slitless spectroscope we have one of the most powerful instruments for the prosecution of eclipse work, it seems doubtful whether it will accomplish all that our author, its warmest advocate, expects from it. It is hoped to get a definitive spectrum of the corona, by means of subtracting from the spectrum of the *whole eclipse*, obtained with an integrating spectroscope, that portion which is due to the chromosphere alone. This latter is to be determined by the prismatic camera. It is not impossible that a line might be common to both chromosphere and corona, but shine so feebly in the latter that its presence would be masked by the continuous spectrum. In such a case the line would be assigned to the chromosphere alone. It would, therefore, seem as though the true solution of the problem is to be expected from the slit spectroscope, part of the slit being made to lie on the moon's shadow In order to make such an attack complete many parts of the corona should be covered. With an instrument of the probable dimensions of that described by Sir Norman Lockyer the field of the collimator should be flat enough to allow several images of the sun to be used. These could be twisted by means of reversion prisms so that any portion of the corona could be brought upon the slit. In this manner the regions surrounding the sun could be well commanded.

It will be seen that in the case of the integrating spectroscope the full efficiency can not be developed, as the central part of the lens will be covered by the dark cone of the moon. Again, and this is more or less in the same line of argument, the brightness of a line will be an average of the brightness of that color over the entire field, while with the instrument provided with a slit we have maxima and minima, which is important in the case of faint lines. For these and other reasons it seems doubtful whether the great power the instrument described might not be used to better advantage in some manner other than the one proposed.

It is expected, by means of the prismatic camera, to decide between the two contending hypotheses regarding the distribution of gases in the solar atmosphere. Do the vapors all rest upon the photosphere, and thin out at different heights, or are they arranged in concentric layers? One of the methods suggested is as follows, to quote Sir Norman Lockyer: "There is a very definite way in which the photographs taken with the prismatic camera may indicate the presence of layers of vapors concentric with the photosphere, but not reaching down to it.

At a certain height above the photosphere the chromosphere spectrum in a photograph of the chromosphere visible at any one instant beyond the edge of the moon will show arcs of certain relative intensities. As the moon advances and gradually uncovers the base of the chromosphere the same arcs will remain visible, but those produced by a layer which does not extend down will be reduced in intensity as compared with arcs produced by vapors which do reach lower down; the latter will continue to get brighter, while the others remain at the same absolute intensity. As the lowest part of the chromosphere is shown in the photographs taken immediately after totality, or exactly at the end, it is only necessary to compare the relative intensities of the arcs in different photographs, in order to investigate the general question as to the existence of layers."

Let us now consider what we should be led to expect under the hypothesis that all the gaseous envelopes rest upon the photosphere. There are no grounds for believing that those

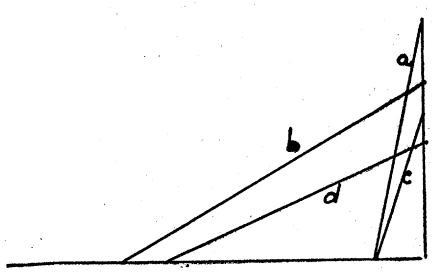

FIG. 1.

gases which extend the highest should be intrinsically the brightest. In fact, we should expect extent and brightness to depend largely upon separate conditions. In Fig. 1 let the ordinates represent the height of a point above

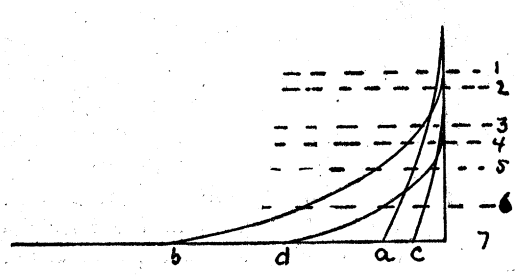

FIG. 2.

the chromosphere, and the abscissæ the brightness of a gas at that point. The lines *a*, *b*, *c* and *d* are supposed to represent the relation between height and brightness of four different gases. For simplicity, and in the absence of definite information upon the subject, these lines are assumed to be straight. If the examination is made close to the photosphere the *effective* intensity of the arc will be proportional to the amount of gas uncovered per unit of length along the moon's edge. Fig. 2 has been roughly sketched to indicate the effective intensity at different levels. Fig. 3 shows the

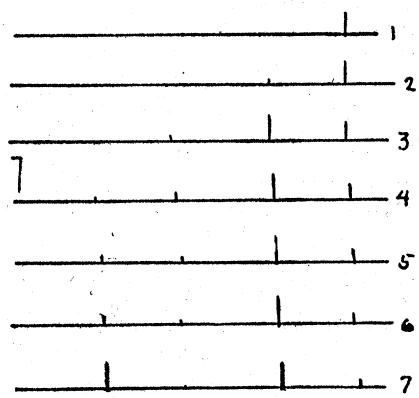

FIG. 3.

relative intensities of the lines at the levels indicated. If the extent of some of the vapors is so great that their arcs have a considerable area the case is still further complicated. If we understand our author aright, a series of photographs corresponding to Fig. 3 would indicate layers *d* and *b* resting on the photosphere, followed by *c* higher up and finally by *a* alone. It is true that a rough scale of *absolute* brightnesses might be built up by comparing the middle of the arc with portions nearer the cusps, but such an arrangement would be only approximate, and is evidently not intended by the author. It is not, therefore, at once apparent that the riddle proposed by the relative intensities of a large number of lines belonging to different gases is easy of solution.

It is with regret that the reader finds throughout the work statements and suggestions to which, perhaps, for a want of comprehension of exactly what is meant, he is forced to issue a

mental challenge. The one just discussed is an example; others might easily be cited, for instance the reasoning leading to the conclusion that prominences are not of the chromosphere and must, therefore, come from the outside. But space forbids further discussion in this direction.

The training of the volunteer corps of H. M. S. 'Volage' was ingeniously planned and carried out with pains. Parties consisting of those fitted for certain classes of work were organized and regularly drilled for some time preceding the eclipse. In training the sketchers, former coronas were thrown on a screen by means of a magic-lantern and, after some practice, remarkable proficiency was shown in accurately drawing the objects, within the eclipse interval of time. It is doubtful, however, whether results of value are to be had from drawings of the corona. Since such very short exposures are required completely to fog a photographic plate the question of getting faint outlying details is merely one of contrast, and with skillful exposure and development there seems to be no reason why the camera should not be considered superior to the sketch-book in delineating eclipse phenomena, as it has shown itself to be in innumerable other branches of research.

With regard to the bearing of solar work in general on dissociation, it is safe to say that the consensus of scientific opinion is not with Sir Norman Lockyer. While dissociation is admitted as a possibility, it is not considered that a preponderence of evidence has given it the standing of a scientific fact. It is claimed that for astrophysics there is laid the foundations of an exact science. But as yet the superstructure has not neared completion. Peculiar characteristics of spectra accompany certain physical conditions. Good work has been done in the direction of associating the one with the other, but it is only a beginning. It is doubtful whether most scientists consider that the influence of all our terrestrial conditions upon the spectrum has been determined, or even guessed, to say nothing of those which may exist in the sun and stars. In time to come, when knowledge becomes more definite on some of these points, and the effect of influences probably existing in the sun has been allowed for, we may, with a mental reservation, assign the residual anomalies of solar and stellar spectra to some condition which we suspect to exist. Until then this line of attack is to be followed with caution.

"In the course of the spectroscopic solar investigations which have been going on since 1868 I have had to point out over and over again that the phenomena observed could be more easily explained on the hypothesis that the chemical elements with which we are familiar here were broken up by the great heat of the sun into simpler forms" etc. In the present state of our knowledge it is somewhat of a problem how much of a figure the question of 'ease' should cut. We call to mind the fact that, on account of insufficient experimental data, the phenomena of light were more *easily* explained to Newton by the emission hypothesis than by the wave theory. And we are not all Newtons.

In closing, however, it is to be said that Sir Norman Lockyer has given us an interesting book, one particularly so to the general public. Technical subjects are explained in simple language, and the mere recital of facts and theories has been relieved from time to time by digressions upon subjects of a more human nature. This is particularly so in the account of the 1896 eclipse expedition. It is hoped that the volume will give to amateurs and others who may witness the coming eclipse such a knowledge of some of the problems awaiting solutions as will enable them to make intelligent observations which may be of interest to themselves and of use to science.

W. H. Wright.

Lick Observatory, December, 1897.

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